Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-254-AC2, 2018

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Factors influencing spring and summer areal snow ablation and snowcover depletion in alpine terrain: detailed measurements from the Canadian Rockies" *by* Michael Schirmer and John W. Pomeroy

Michael Schirmer and John W. Pomeroy

michael.schirmer@slf.ch

Received and published: 13 December 2018

We thank the reviewer for the constructive comments. We copied the reviews comments in this reply for a better readability and marked them using italic fonts.

The study of Schirmer and Pomeroy used high-resolution aerial photographs of a mountain ridge to determine the spatial distribution and height of the snowpack (HS) during the melt season. Several surveys were undertaken at different times and the differences in snow height measurements (dHS) were used as proxies for ablation. The

spatial patterns of ablation (dHS) was compared with pre-melt snow water equivalent (SWE, measured manually) and several topographical variables. Albedo (i.e., brightness of the snow) and solar radiation differences (i.e., deviation from North or soral irradiance) were identified at the dominant controls on dHS, whereas there was no correlation between dHS and initial SWE. The authors explain this lack of correlation with the difference in spatial scales at which dHS and initial SWE are affected by topographic and climatic variables. The high-resolution measurements of dHS further allow to estimate the spatio-temporal variability of ablation. The authors find that the variability in ablation (dHS) is much smaller than that of initial snow depth (HS0). Consequentially, snow cover depletion curves (SCD) are less sensitive to the spatiotemporal variability of ablation and most sensitive to the HS0 of the area. The authors show this by determining and comparing SCD's from combining either uniform or variable initial HS0 with uniform or variable dHS.
The high-resolution data set of spatial snow depth distribution is unique and potentially allows interesting analyses, however, I find it difficult to identify the novel scientific contribution of this study. One of the main findings, that initial snow depth (HS0) is not correlated with changes in snow depth over time (dHS) is only briefly mentioned in Sect. 3.3.

This and finding an explanation for the lack of correlation is a focus in the revised manuscript.

The second finding, SCD curves for the study area are largely affected by HS0 and less by dHS, has been studied extensively previously (P2L21-24, P3L31-32). I would thus recommend to revise the manuscript in a way that brings out the novelty of the authors' findings more clearly and to help the reader to learn something.

The novelty is given with the high-resolution data set. Since snow depth is known to vary largely below a scale break of tens of meters in alpine environments we think that a replication of studies which relied on much coarser manual probing is needed. Manual probing with a spacing at or larger this scale break may complicate the interpretation

of the results (Clark et al., 2011), since the dominant spatial structure can hardly be captured. A high resolution data set was presented by Grünewald et al. (2010) and Egli et al. (2012), but only covered one study region and two different seasons. Testing of these studies in other areas is urgently needed to show the transferability of results. Furthermore, we present an explanation on the missing correlation between SWE and melt. This is now more precisely mentioned in a more focused way in the revised manuscript.

In addition, the language of the manuscript needs to be improved as some sentences are confusing and hard to understand (e.g., p11L27: "However, no study showed consistent and persistent fine-scale association between ablation and SWE suggested that they can be considered uncorrelated in modelling at fine scales."). Please find some more detailed comments below.

We have revised the language throughout the manuscript.

Introduction:
P2L30 – P3L33: It is difficult to follow the authors' train of thought here as this paragraph seems like a random collection of studies without an overarching theme that help the reader to get to the same conclusions as the authors. Is the overall point of this paragraph to show that SWE and melt are variable over time and space or that it is challenging to determine an accurate SCD curve? If so, it would help to state this as a theme at the beginning of the paragraph.

We have re-written the introduction to focus on studies with minor vegetation effects on snow depth distribution and on found correlations between SWE and melt.

Please explain briefly what a SCD curve is.

Done.

Methods: 2.1 Site description: Why was this study site chosen, given that the snow distribution was strongly affected by ski slopes and strong winds?

We will write a better explanation on why we chose this study area. The strong winds were the attractive point of this location. The skiers influence was small and spatially limited so that impacted zones could be excluded in the study area.

"A study region was chosen which showed substantial differences in aspect and slope to ensure spatial melt differences. In a nearby study site DeBeer et al. (2010) found spatial melt rates to be important for snow cover depletion, at least during early melt. Large drifts in southerly parts of the ridge commonly form in this area suggesting a correlation between melt energy and SWE."

2.2 UAV Data acquisition: please don't use abbreviations in the headings or write as "Unmanned aerial vehicle (UAV) data acquisition"; How many flight were made in total? Can you please provide the dates of the individual flights in this section?

Done as suggested. The UAV was flown 18 times over snow from 15 May to 24 June 2015 at eight different days with substantial depth differences between these days and four flights over bare ground on 24 July 2015. However, as stated in the manuscript, we had to restrict analysis to two melt periods.

Your statement on p5L20 is not clear enough: "Ideally, four flights in total were made each sampling day, two for each subarea with perpendicular flight plans. Weather conditions and technical problems often allowed only a part of this program."

We will clarify this topic and will mention e.g. how many flights per day were done in average, why perpendicular matter, why subareas were defined, and what weather conditions and technical problems restricted the surveys.

2.3 Accuracy evaluation and manual measurements: From this description of the methods I understand that for each (4?) sampling day, snow depth and density (i.e., SWE) were measured at up to 7 locations over the entire field site. Were these SWE estimates assumed to be representative for the times between measurements? Did you multiply snow density with dHS to estimate the ablation rate? This is not at all clear

from your statement: p6L5-9 "At these GPS measurement points, snow depth was also manually measured and snow density was measured at approximately each third of these points. Density measurements were not sufficient to confidently estimate SWE from snow depth into SWE and ablation rates from differences in snow depth. As such the originally measured quantities are analysed and interpreted as proxies for ablation and SWE in the text." Also, what do you mean by "originally measured quantities"? Please be more precise.

We have clarified this in the revised manuscript. We did not multiply dHS with snow density to estimate the ablation rate, because we think that our SWE measurements were neither representative for the whole area nor for the time in between the measurements. This is why we analysed and interpreted HS and dHS as proxies SWE and ablation rate.

Results and discussion: 3.3 Spatial differences in dHS: It would be nice if you could also provide the correlation results for the remaining variable Slope, as well as the p-values for all correlations, for completeness (Table 1).

We have included slope. P-values are not very meaningful for these large numbers of observations since statistical significance is almost always achieved – this is now noted.

In Section 3.3. you use dHS (change in snow depth) equivalent to melt (or ablation), although nowhere before was explained what this assumption is based on and how melt was estimated. This important bit of information only comes later (Sect. 3.4, P12L28-30); please include this description into the Methods section. Also, if you simply multiply HS and dHS with a uniform and temporally constant snow density, the variability of the resulting SWE values and melt volumes are the same as for HS and dHS multiplied by snow density.

We used the words melt and SWE for a better readability in the original submission, lines P12L28-P13L2. However, in the revised manuscript we will only use the "words"

dHS and HS.

P11L29-32: Your main finding, that is that initial HS is not correlated with dHS, is somewhat hidden in section 3.3. Given that this is a major result of your study, I would suggest to include a figure similar to Fig.5 that actually shows this lack of correlation.

This is done as suggested.

Also, your conclusion "These values indicate much larger SWE variability than ablation variability in this period." is equivalent to a larger variability of HS relative to dHS (in other words: the relative standard deviations of HS and dHS are same as for SWE and melt volumes). Thus, it seems confusing to use SWE instead of HS and melt instead of dHS, because SWE and melt are not measured the same way as HS and dHS.

In the revised manuscript we only use HS and dHS.

Minor comments:
- P4L5: What results of what models? A reader not familiar with snow hydrology literature has no idea what it meant by that.

We have re-written the introduction to address this.

- P6L18: Please explain what SfM means.

The abbreviation SfM was explained in P4L15.

- P11L24: "The correlation of. . ." What? ". . . with. . ."

This is included in the revised manuscript. The correlation of dHS with . . .

- P16L22: ". . .varying exposures of vegetation, which is not a factor in this study." Earlier in the manuscript you state that vegetation has a strong effect on snow distribution. Please explain.

We have indeed written in P4L29ff that vegetation had an effect on snow distribution, but that these areas were exluded from the analysis. We will change the wording to

clarify this topic.

- P7L27: Shouldn't it be "decrease of R2"?

Yes, many thanks for finding this error.

- P12L23: "Relative importance of ablation and initial SWE" Relative importance for what?

Many thanks, we will change this into Relative importance of dHS and HS0 on snow cover depletion.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-254, 2018.